INSTITUTO DE ECONOMÍA

DOCUMENTO de TRABAJO

513
2018

Followed by Violence: Forced Immigration and Homicides

Emilio Depetris-Chauvin y Rafael Santos.

www.economia.puc.cl • ISSN (edición impresa) 0716-7334 • ISSN (edición electrónica) 0717-7593

Followed by Violence: Forced Immigration and Homicides

Depetris-Chauvin, Emilio & Santos, Rafael J. *

We study the causal impact of large inflows of internal displaced people (IDP) on homicides in Colombian host municipalities during the period 1999-2014. Following two distinct instrumental variable approaches while leveraging on high quality and high frequency administrative panel data on IDP flows and homicides across Colombian municipalities, we identify an economically sizable impact: a standard deviation increase in inflows, increases the homicides rate by 0.6 standard deviations. This effects is larger in cities and among men. While IDP inflows are associated with increasing homicide rates for all the age groups, we document that the standardized effects are larger for young individuals (i.e., age groups 15-19 and 20-24 years old).

JEL: J1, O15, R23, R31

Keywords: Internal Displaced People, Migration, Violence, Crime.

* Depetris-Chauvin: Pontificia Universidad Católica de Chile (edepetris@uc.cl) ; Santos: Universidad de Los Andes and CEDE (rj.santos@uniandes.edu.co). Edgar Sanchez provided outstanding assistance with this research. Emilio Depetris-Chauvin acknowledges financial support from CONICYT, FONDECYT Iniciación 11160290.

Economics has long studied the effects of immigration on the labor market (Grossman, 1982; Card, 1990; Borjas et al., 1997). However, politicians, the media and the layman seem more concerned about the impact of immigration on crime. Gallup polls in the US —the so-called nation of immigrants— reveal that 45% of respondents think that immigrants make the crime situation worse while 28% believe that immigrants reduce job opportunities.[1] In Germany, refugees are blamed for the recent increase in violent crimes. In Colombia, one of the main concerns of city residents regarding internally displaced people, IDP from now on, is that they might increase criminality.

This paper seeks to understand the reality behind that fear of immigration. We seek to understand the effect of IDP on homicides in host municipalities within Colombia. Colombia is an important case study. As of 2016, no other country has experienced internal displacement figures as large as in Colombia: The Internal Displacement Monitoring Center reports a head count of 7246000 individuals displaced by violence (or 14.9% relative to population).

A priori, there are theoretical reasons to believe that forced immigrants might increase crime. Indeed, since IDP were exposed to violence —and given research that indicates that exposure to violence engenders violent behavior (Gutierrez (2016), Couttenier et al. (2016))— one might expect IDP to be more likely to engage in crime compared to the rest of the population. Two other factors might increase the propensity of IDP to engage in criminal activity: their lack of economic opportunities and selectivity (IDP are younger, more likely to be male and have low levels of education as described in Ibáñez (2008)). From a different perspective, criminality of all

[1]See, http://news.gallup.com/poll/1660/immigration.aspx.

actors in reception sites might increase. Previous work has shown that IDP inflows reduce unskilled wages (Calderón-Mejía and Ibáñez, 2015) while they also increase low-income rentals in host cities (Depetris-Chauvin and Santos, 2018). To the extent that these factors deteriorate economic conditions, we should expect increased criminality from both IDP and non-IDP (Becker, 1968; Ehrlich, 1973). IDP inflows might increase crime, in part because they temporarily disrupt the functioning of markets.

To proceed with this research we merge data on homicides with quarterly frequency and cross-sectional variation at the municipality level with hight quality data on IDP inflows. Given its quality, high frequency, and temporal extension, these data provide a meaningful source of variation in IDP inflows to identify their effects on crime. Indeed, unlike previous works that —due to the lack of IDP data at the host level— use different proxies for the intensity of IDP inflows, our paper exploits actual IDP figures at the municipality level (1122 municipalities). Further, as the intensity of the conflict varied over time and across the Colombian territory, our panel data analysis allows us to account for the fact that the intensity and timing of these large rural-urban migration flows also varied across municipalities. Not less important, the time period under analysis provides time windows before, during, and after one of the the most dramatic displacement crisis which ocurred at the turn of the century (see also Depetris-Chauvin and Santos (2018)).

We first present OLS results on the relationship between the homicides rate and inflows. Conditional on quarter and muncipality fixed effects, together with municipality time trends and interactions between time dummies and a measure of geographical remoteness, our preferred specification suggests a positive and highly significant association between inflows in $t-1$ and homicides in t. However, this correlation might be contaminated with endogeneity, in particular because we cannot control perfectly for economic prosperity in recipient municipalities.

To deal with this endogeneity, we follow an instrumental variables approach. Our instrument (to which we refer as "Receptivity") captures the potential of a city to receive IDP. The instrument is the weighed sum of IDP outflows from all municipalities except the receiving host municipality, where the weights are (the inverse of) the road distance between the host municipality and the municipality where the IDP outflow originates (this instrument was first introduced by Depetris-Chauvin and Santos (2018) but using a small sample of 13 host cities). Estimations with the IV-Receptivity instrument indicate that a one percent increase in inflows, increases homicides by 0.071% and this effect lasts for at most five quarters (the corresponding standardized coefficient is equal to 0.56 standard deviations). Nicely, we show that our results are almost identical if we use an Enclave instrument which relies on the importance of previous migration inflows and immigration networks. By presenting results with this alternative instrument we join the vast literature which proceeds likewise (Card, 2001; Saiz, 2007; Morales, 2018) . We then provide more details or refinements about how the effects of IDP on homicides work.

The most important contribution of this paper is its novelty: It is the first paper to analize the relationship between forced migration and crime, which is also a highly policy relevant topic. The paper most similar to ours is Varano et al. (2010) which studies the impact of migrants forcefully displaced by Hurricane Katrina on crime in Houston, San Antonio and Phoenix. With the caveat that they only have time series data and no control groups, they find that homicides increased in Houston and Phoenix, a result that echoes ours. This paper also speaks to the literature of immigration and crime which is also very narrow. We could only find two papers that tackle this issue and both focus on developing countries (immigration to Africa and Latin-America represents 13.6% of world immigration (United Nations, 2017) and developing countries hosted 86% of the global refugee population (UNHCR, 2014 Yearbook)). Bell et al. (2013) studies the effect of two immigration waves into the UK (one of refugees and one of workers) finding an effect on property crimes but no effect on other crimes. Similarly, the work of Bianchi et al. (2012) finds that migrants into Italy increase robberies but no other crimes.

The remainder of the paper is organized as follows: section I provides the context of displacement and characterizes national trends in Colombian homicides. Section II presents the data and section III the econometric model. Section IV presents our main OLS and IV results and section V further analyzes the details behind the effect of forced migration on homicides. Section VI concludes.

I. Context: Forced Displacement and violence in Colombia

Colombia suffers from a long history of forced displacement as a result of political and drug-related violence. Left-wing guerrilla groups, like the FARC and ELN, emerged in rural areas of the country in the 1960s and persisted for years engaging in relatively low-scale violence against the Colombian government. Levels of violence began to increase in the 1980s and 1990s as these rebel groups entered the drug trade. During the mid-1990s and early-2000s, right wing paramilitary groups stepped in to fill the void created by the lack of State presence in many parts of the country, themselves relying on the narcotics trade for their financing. Colombians in peripheral and rural areas were caught in the middle of a three-way war between paramilitary groups, guerrillas, and government forces (as described in Depetris-Chauvin and Santos (2018)).

In this context, millions of civilians were forcibly displaced from their homes for a variety of reasons. Since the beginning of the conflict, the United Nations High Commissioner for Refugees (UNHCR) estimates that over 6,640,000 individuals have been forcibly displaced from their homes, approximately 15 percent of the Colombian population (UNHCR, 2015).

Though displacement is violent and traumatic, displaced households do not travel far from the expelling municipality. More than half of displaced households reestablish themselves within the same department, an administrative division similar to the state in the USA (Ibañez, 2008). In contrast with other countries, there are no displacement camps in Colombia.

How do displaced individuals/households compare to the rest of the population? Data in Ibáñez (2008) help us answering this question. The most obvious difference is that displaced households have been exposed to higher levels of violence. This is important given evidence that individuals exposed to violence are more prone to violent crimes (Couttenier et al., 2016). In addition to that, the level of education of displaced households is lower: 7.4 years versus 9.0 years for non-displaced households. Displaced households are poorer. They also have younger household heads who are usually male. In sum, socioeconomic conditions of displaced people make them more vulnerable to engage in crime (Becker, 1968; Ehrlich, 1973). This last point is aggravated because income and consumption fall for displaced households as a result of displacement. One figure might summarize the economic precarity of displaced individuals: Before displacement the unemployment rate of household heads is 1.7%; after displacement that rate increases by an order of magnitude, to 16.1% (Ibáñez, 2008). There is also evidence that IDP inflows decrease employment opportunities for the unskilled (Calderón-Mejía and Ibáñez, 2015) and increase the price of low-income housing (Depetris-Chauvin and Santos, 2018) in host cities. These outcomes, a result of competition for resources that become scarcer, might increase the propensity to engage in crime of both the displaced and the non-displaced.

Indeed, case study evidence for the city of Pasto, the state capital of Nariño, suggests that security conditions deteriorate in places with settlements of displaced population (Bohada, 2010). In San Cristobal -a locality in eastern Bogotá composed mainly of poor (strata 1 and 2) households- the NGO CHODES reports that "behind the violence in the locality, there is a serious problem of coexistence with the displaced people who arrived in recent years." (Valenzuela, 2014). Perceptions of crime are also affected: In Bogotá, perception surveys reveal that one of the main concerns of strata 5 and 6 residents regarding IDP is increasing insecurity (Departamento Administrativo de Planeación Distrital (2004)).

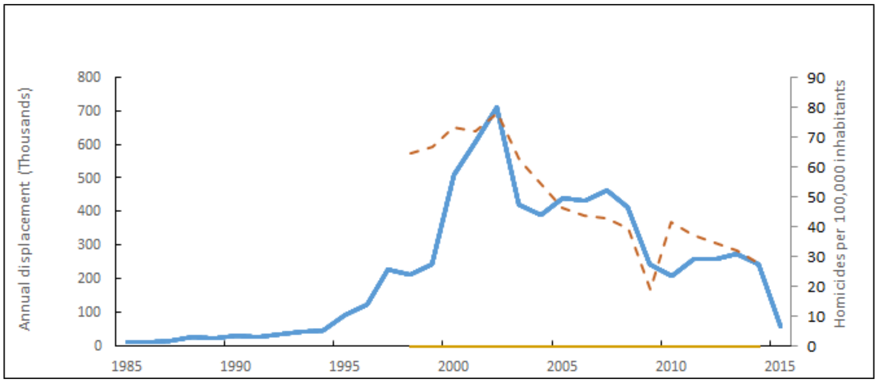
Figure 1a shows the trends in forced displacement for Colombia since 1985 (solid line) and the homicides rate since 1999 (dashed line). The figure shows a notable increase in the number of individuals displaced beginning in the mid-90s, as guerrilla groups ramped up their operations in earnest, and drug revenues fueled the rise of paramilitary groups. The 2000-2001 spike stands out, corresponding to the breakdown of failed peace negotiations with the FARC, and an important period of violent expansion for the AUC paramilitary group (see Acemoglu et al. (2013)). The dramatic decline in displacement after the AUC's demobilization in 2008 is noteworthy, as is the drop since the 2012 announcement of new peace talks between the FARC and the Colombian government (see also (Depetris-Chauvin and Santos, 2018)). The evolution of homicides follows closely the evolution of displacement, which just reflects the murderous nature of Colombian conflict: the homicides rate fell from 80 in the early 2000s to 30 in 2015. What we are after is the fraction of the murders rate that is explained by IDP inflow in recipient municipalities, which of course, should be much smaller than the actual murders rate.

Figure 1b shows the evolution of the homicides rate for municipalities that receive inflows above the median between 1985 and 1999 (regular IDP recepients, solid line) and those that receive inflows below the median (non-regular recepients, dashed line). From the figure it is evident that homicides are higher for regular recipients although the absolute value of the difference has decreased over time concomitantly with the decrease in displacement. This figure does not imply however that displacement inflows cause crime; recipient municipalities are very different than non-recipient municipalities. The latter are rural and more likely to be in the periphery of the country.

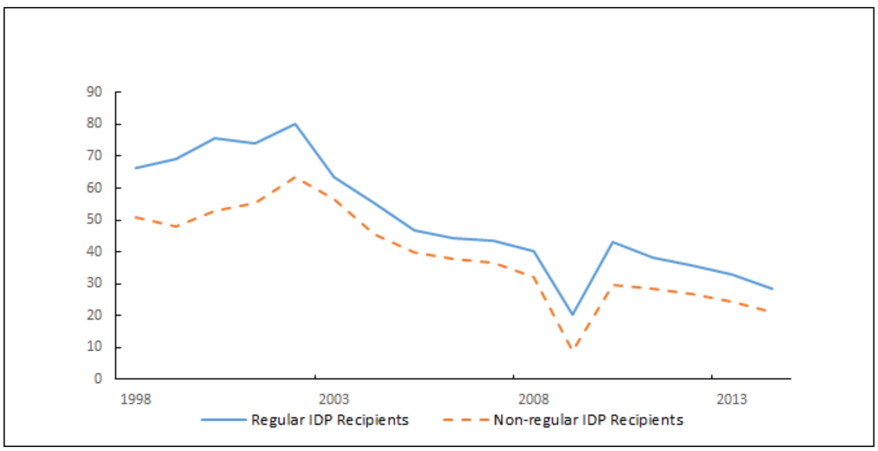
Just as the intensity of displacement has been uneven across time, it has varied across the different regions of Colombia. The left panel in Figure 2 provides information on the intensity of inflows of IDP by municipality (left panel). This intensity measure is calculated as the ratio of accumulated inflows over the 1999-2015 period to the population of the municipality in 1999. As the graphs show, in some cases, IDP represent between 10 and 28% of the municipality's population (this is particularly true for cities).

Figure 1. : Displaced People and Homicides in Colombia

(a) IDP (solid line) and Homicides (dashed line)



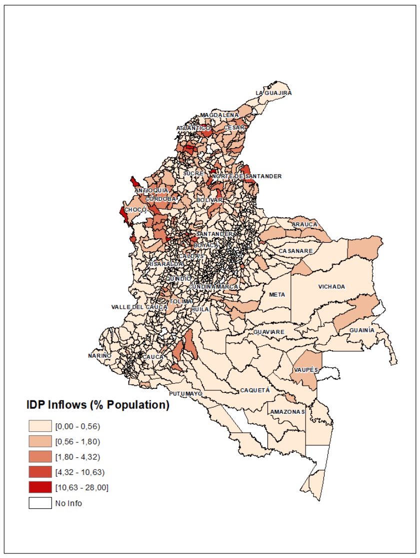
(b) Homicides in regular (solid line) and non-regular (dashed line) IDP Recepients



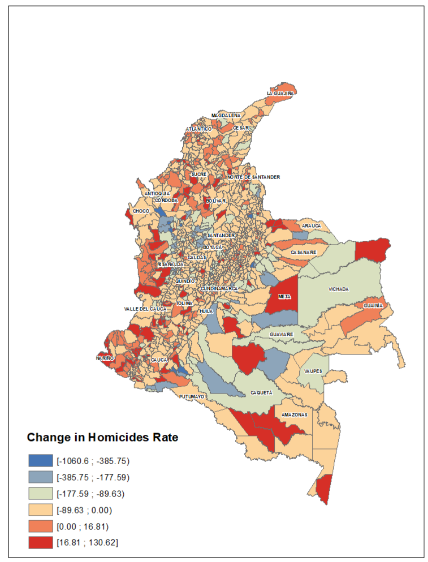
The right panel in Figure 2 shows a map with the change in the homicides rate between 1999 and 2015 by municipality (labels on the map represent departments). Interestingly the departments in the north of the country (Atlantico, Antioquia, Cordoba, Choco, Bolivar and Norte de Santander) received large inflows of IDP relative to population and they also exhibit important increments in the homicides rate. This graphical relationship anticipates our formal results.

Figure 2. : IDP Inflows Intensity

(a) Total Inflows in the period 1999-2014 over population in 1999



(b) Change in the Homicides Rate 1999-2014



In 1999, the Colombian government created a victims' registry, allowing displaced individuals (including those displaced before 1999) to come before a government office, where their displacement status is validated and they can become eligible for receiving government assistance.[2] The information from

[2]There are three assistance categories for the forcibly displaced population: immediate assistance, including temporary housing and food aid, may be provided by the host municipality starting the moment the victim makes their claim until the RUV has made a decision on their case (up to two months); short- or medium-term emergency aid is provided by the RUV to displaced individuals whose cases are determined to meet certain urgency requirements, and allows monthly payments of up to 1.5 times colombian minimum monthly wage; finally, transition assistance in the form of employment programs or access to food or housing assistance is provided on a case-by-case basis to displaced individuals whose cases are not determined to meet the emergency assistance requirements (Ley 1448, 2011; Prada and Poveda, 2012). However, the reality of how this is implemented

the victims' registry is one of the main data sources used in this research.

II. Data

To conduct this research we use different data sources. Most of the variables used in the paper are summarized in Table 1. Appendix Table A.1 provides more details on the source and the construction of the variables. Our main dependent variable is the homicides rate per 100,000 inhabitants which is contructed from Colombian Vital Statistics as collected by the Colombian National Statistics Department (DANE). Colombian Vital Statistics provide information on cause, location, and circunstance of death (including homicides and natural deaths) as well as valuable information of the defunct such as residence, gender, and age. To compute different death rates we use municipality-level population projections based on National Census of 1993 and 2005 from DANE.

Our main independent variable is the log of IDP inflows arriving to city c in quarter $t-1$. We built these data using information from the Registro Unico de Victimas (RUV), a dataset of IDP inflows and outflows collected by the Colombian government registry for IDP (i.e; Registro Nacional de Información -RNI-). The flows that are captured are those arising from the Colombian internal conflict as described in the previous section. From these data we will use outflows in all other municipalities to construct our main instrumental variable, described in the next paragraph. Table 1 shows that on average, the number of IDP inflows is approximately 34 migrants per quarter (from $exp(3.51)$). It is important to note that there is a large

is far from ideal. For displaced individuals who registered between 2002 and 2004, only 50 percent had received any assistance at all, and for much of the period in our study, most qualifying individuals received an assistance package for only three months (Human Rights Watch, 2005).

variance in this measure. For instance, this figure ascends to 1164 migrants per quarter when we focus on the 13 largest cities over the same time-period.

The main instrumental variable is a distance-weighed sum of IDP outflows in all municipalities but municipality m, where the weights are the inverse of the road distance between expelling municipality μ and host municipality m. We label this variable "Receptivity Instrument" as it predicts the potential of a city to attract IDP. The road distance measure is novel in the Colombian context. To create it we relied on road maps from SIGOT-IGAC for the year of 2011, the first year for which we could find an almost complete geo-coded network. Some distant municipalities are not connected to the Colombian road network and this might generate noise in the instrument. More precisely, 111 municipalities of the 1122 for which we have data on outflows are not connected to the road network. Outflows from these municipalities represent 10.2% of all outflows. However, as we will show, results are similar when using an alternative instrument that uses information on outflows from all municipalities. Furthermore, a regression of the probability of being connected to the road network against past inflows produces insignificant estimates, which reduces concerns about selection.

The main regressions in the text include city and time fixed effects, city time trends and interactions between time dummies and the average road distance to all other municipalities (our measure of remoteness), but no additional controls. Nonetheless, for robustness purposes, we also include in other specifications determinants of homicides. We control for population and the share of the population between 15 and 39 years of age (both variables from DANE), for the fraction of the population that lives in rural areas, for tax revenues from industry and commerce as a proxy for economic activity and for the number of public school teachers per students as a measure of amenities (all from CEDE, at Universidad de Los Andes). We also use unemployment rates in 1993 (from DANE's national census) and the number of military Bases in 2007 (from Acemoglu et al. 2016); two variables measured in the pre-treament period that we interact with time dummies.

Table 1—: Descriptive Statistics

	mean	sd
Homicides Rate	12.278	23.812
IDP Inflows	2.350	1.921
Receptivity Instrument	0.174	0.090
Enclave Instrument	3.519	1.337
Average Road Distance	13.152	0.512
Population	9.589	1.089
Tax Revenues	4.143	2.375
Public school teachers per student	0.044	0.010
Rurality Index	3.892	0.737
Unemployment Rate in 1993	0.779	0.867
Share of population between 15 and 39	3.643	0.074
Military Bases in 2007	0.107	0.372

All variables expressed in natural logarithms but the homicides rate and military bases.

III. Econometric Model

A. OLS

Our objective is to retrieve the causal effect of IDP inflows on homicides in host municipalities. Using panel data with quarter frequency at the municipality level, we start by estimating naif regressions where inflows are assumed to be exogenous. That is, we estimate:

$$H_{m,t} = \alpha + \beta ln\left(Inflows_{m,t-1}\right) + \eta^{'} X_{m,t} + d_m + d_t + u_{m,t}, \tag{1}$$

where the subscripts m and t denote municipality and quarter, respectively. The variable H is the homicides rate per 100,000 inhabitants. $Inflows$ is our main independent variable and represents the total number of displaced people arriving to the host municipality m during the time period (quarter) $t-1$. The linear-log specification generates $\hat{\beta}$ estimates that are interpreted as follows: one percentage change in IDP inflows changes the homicide rate by $\hat{\beta}/100$ percentage points. d_m and d_t are municipality and quarter fixed effects. $X_{m,t}$ is a vector of controls. Our main controls are two: time trends by municipality and time dummies interacted with remoteness measured as the average distance to all municipalities other than m.

In some specifications we gauge the robustness of our results to an expanded set of controls. The expanded set of controls includes important determinants of crime. While these controls might be endogenous, we include them to assess the sensitivity of our main estimates. We control for population, which also controls for population density given the fixed effects at the level of the municipality, as well as for the fraction of the population in the age range 15 to 39 because young men are more prone to engage in criminal activity (these controls follow closely the existent literature (Bianchi et al., 2012)). Bringing to mind traditional models of crime (Becker, 1968; Ehrlich, 1973) we account for the potential effect of economic activity (proxied by tax revenues of industry and commerce) and unemployment. For the latter we use pre-treatment values (i.e., measured in 1993) interacted with quarter dummies which alleviates endogeneity concerns. We also control for the quality of the school system —which might deter students from crime— as measured by the number of public school teachers per student and for an index of rurality (rural population over total population in the municipality). The expanded set of controls also includes interactions between quarter dummies and the count of military bases in the municipality in 2007.

To finish describing equation (1), $u_{m,t}$ represents an error term clustered at the municipality level. Finally, we weight all the regressions by population because the effect of IDP inflows is larger in bigger cities. In bigger cities, competition for scarce resources (for example, housing) seem to be more severe (see Depetris-Chauvin and Santos (2018)).

We focus on IDP inflows lagged one period for two reasons: First, analyses of the lag structure below suggest that inflows in t-1 are the most important determinant of homicides; second, using a lagged independent variable may reduce concerns of reverse causality between IDP inflows and homicides.

Of course, this approach of lagging the inflow variable does not convincingly solve potential endogeneity problems. According to the 2004 survey of IDP (Centro de Estudios sobre Desarrollo Economico, 2004), 15.28% of displaced households leave their origin because of economic reasons. This means that push factors (violence in the origin) are more important than pull factors (economic conditions in the city of arrival). However, 15% is not a negligible number. Furthermore, it is hard to argue that, after being displaced by violence, IDP chose their destination randomly.

If inflows are correlated with time varying characteristics of the recipient city, OLS estimates might be biased. For example, IDP might move to cities with better economic prospects (not captured by our controls). Since economic upturns reduce crime, this would bias OLS estimates towards zero. Additionally, we cannot dismiss potential bias from measurement error in our

IDP measure. In this sense, attenuation bias could be particularly important given that we exploit a panel data where fixed effects at both the city and quarter level are included.[3]

B. IV

To deal with the endogeneity of inflows we use two different instruments. Our main instrument, which we label $receptivity_{m,t}$, accounts for the intensity of IDP outflows generated in each Colombian municipality every quarter during the period 1998-2014. This measure is a road-distance-weighed average of the outflows in all municipalities except municipality m during quarter t. Formally:

$$receptivity_{m,t} = \sum_{\mu \in M} outflows_{\mu,t} \times D^{-1}_{m,\mu}, \tag{2}$$

where $m, \mu \in M$ are municipalities in our sample. $D^{-1}_{m,\mu}$ is the (inverse of the) road distance between the recipient municipality (m) and the expelling municipality (μ). The instrument suggests that the number of IDP arriving to m in time t increases with the number of outflows in other localities, but decreases with the distance from any expelling locality to the municipality. Thus, and because the majority of IDP move to final destinaton in less than a quarter (Centro de Estudios sobre Desarrollo Económico, 2015), the log of receptivity in $t-1$ is used as an instrument for the log of inflows in $t-1$.

This instrument is based on two ideas. First, the timing and intensity of violent events that engender displacement are arguably orthogonal to relevant characteristics of the host municipalities. Second, the closer the proximity of

[3]It can be shown that under the case where measurement error is serially uncorrelated, using a fixed effect model might increase the variance of the measurement error while it might reduce the variance of the signal thus worsening the original attenuation bias.

a host municipality to a municipality experiencing IDP outflows, the higher the probability of receiving large IDP inflows for that host municipality. Indeed, using the 2004 survey of displacement (Centro de Estudios sobre Desarrollo Economico, 2004), 45.45 % of displaced households chose their destination because it is close to the origin.

The exclusion restriction is that receptivity only affects homicides through the channel of IDP inflows. One important concern is that receptivity might be capturing the remoteness of a city. For this reason, the majority of our regressions control for interactions between time dummies and remoteness. Another concern is the spatial correlation of violence: if violence in nearby municipalities is correlated with violence in the host city, our identification strategy might be invalid. To deal with this potential threat to identification, we conduct a series of robustness checks where we exclude outflows from nearby municipalities (see Section IV).

To join the existent literature, we also make use of an additional instrumental variable which is based on the importance of pre-existent networks in determining migration decisions of posterior forced migrants. This "Enclave" instrument is constructed by replacing $D_{m,\mu}^{-1}$ in (2) by $I_{m,\mu}$, the fraction of immigrants from municipality μ in m in 1993 (the denominator is population in 1993). According to the 2004 survey of displacement (Centro de Estudios sobre Desarrollo Economico, 2004), 78% of displaced households chose their destination because they have families and friends.

Thus, a priori, we should expect a higher first-stage F when using the enclave instrument but an important concern remains: The factors that made people migrate to m in 1993 might be part of the error term in (1) because they persist over time. The enclave instrument is subject to the criticism of persistence in the determinants of migration. The receptivity instrument is subject to the criticism of the spatial correlation of violence. To the extent that both instruments do not completely wash out endogeneity, we can interpret them as complementary. However, the receptivity instrument has the advantage that it is easy to modify in order to take into account outflows from relatively distant municipalities only. In that sense, we believe that receptivity does a better job in solving endogeneity biases.

IV. Main Results

We start by looking at the correlation between homicides and inflows conditional on municipality and quarter fixed effects. Table 2 column 1 show the parameter estimate of this regression. The parameter estimate of interest is highly significant —at less than the 1% level— and equal to 1.29. Thus, an increase of 1% in inflows, increases the homicides by 0.0129 per 100,000 inhabitants (i.e., 10% of its mean value). In the bottom of the table we report standardized coefficients, an alternative way of assessing the magnitude of the estimate. In column 1, a standard deviation increase in inflows, increases the homicides rate by 0.1 standard deviations. Column 2 adds municipality time trends. The parameter estimate of inflows remains virtually unaltered and the standard error becomes even smaller than in the previous column. Column (3) adds trends by remoteness and column (4) the expanded set of controls but the parameter estimate remains almost unchanged and become more significant in each new column. These OLS regressions point to a robust and positive conditional correlation between IDP inflows and crime as measured by homicides. However inflows might still be correlated with the error term. For instance, none of our measures of economic opportunities (taxes from industry and commerce and unemployment in 1993 interacted with time dummies) are ideal and they might not reflect the prospects of growth in a city. This omitted variables problem biases OLS estimates towards zero if criminality is negatively correlated with economic prosperity. Further, measurement error in our main independent variable may generate an additional source of bias toward zero. For this reason we turn the attention to our Instrumental Variables results.

Table 2—: Homicides and IDP Inflows. OLS.

	(1)	(2)	(3)	(4)
IDP Inflows t-1	1.293***	1.304***	1.291***	1.460***
	(0.314)	(0.281)	(0.275)	(0.225)
Observations	65467	65467	65467	58183
Standarized Coefficients	0.103	0.104	0.102	0.114
Municipality FE	Y	Y	Y	Y
Time FE	Y	Y	Y	Y
Municipality-specific Linear Trend	N	Y	Y	Y
Time Dummies Interacted with Remoteness	N	N	Y	Y
Expanded Set of Controls	N	N	N	Y

Standard errors clustered at the municipality level in parentheses. The dependent variable is the homicides rate per 100,000 inhabitants. All non-dichotomous and non-count variables expressed in natural logarithms. All regressions are weighed by population. Expanded set of controls: population, tax revenues from industry and commerce, public school teachers per student, percentage of the population aged 15 to 39, rurality index, plus the unemployment rate in 1993 and the count of military bases in 2007, both time interacted.

Table 3—: Homicides and IDP Inflows. IV using Receptivity

	(1)	(2)	(3)	(4)
IDP Inflows t-1	2.833**	6.672***	7.129***	8.870***
	(1.333)	(1.731)	(1.926)	(1.934)
Observations	65467	65467	65467	58183
Standarized Coefficients	0.225	0.530	0.566	0.690
F-Statistic	236.4	96.44	72.82	92.89
Municipality FE	Y	Y	Y	Y
Time FE	Y	Y	Y	Y
Municipality-specific Linear Trend	N	Y	Y	Y
Time Dummies Interacted with Remoteness	N	N	Y	Y
Expanded Set of Controls	N	N	N	Y

Standard errors clustered at the municipality level in parentheses. The dependent variable is the homicides rate per 100,000 inhabitants. All non-dichotomous and non-count variables expressed in natural logarithms. All regressions are weighed by population. Expanded set of controls: population, tax revenues from industry and commerce, public school teachers per student, percentage of the population aged 15 to 39, rurality index, plus the unemployment rate in 1993 and the count of military bases in 2007, both time interacted.

Appendix Table A.2, column 1, shows the first stage using the Receptivity instrument for our main specification, the one that includes time and municipality fixed effects, municipality time trends and interactions between remoteness and time dummies but no additional controls (i.e.: Column (3) of Table 2). Our Receptivity instrument has strong predictive power: the first-stage F is equal to 72.82. Table 3 show the instrumental variables results using this instrument. Table 3 is directly comparable to table 2 (the same columns include the same independent variables). All regressions produce positive and significant estimates. Column (1) —which conditions on municipality and time fixed effects— indicates that a 1% increase in inflows, increases homicides per 100,000 inhabitants by 0.028. However, the evolution over time of host-municipality-specific variables might bias the estimate in column (1) towards zero. This would be the case if remote municipalities experienced an increase in the presence of non-state armed actors, if

Receptivity is lower for remote municipalities and if homicides are positively correlated with the presence of these actors (as it is the case in Colombia). Column (2) deals with this problem by controlling for municipality specific time trends. Column (3) adds interactions of time dummies and remoteness. Column (2) produces a $\hat{\beta}$ equal to 6.672, significant at the 1% level. Column (3) produces in turn a $\hat{\beta}$ equal to 7.129, significant at the 1% level. These results suggest that municipality time trends might be enough to capture the evolution over time of factors omitted in equation (1).

The IV estimates are almost 4 times the OLS estimates, consistent with the bias that can arise from unmeasured economic prosperity and measurement error in our IDP variable. Standardized coefficients are large and equal to 0.5. When we include the expanded set of controls, the parameter of interest increases by 24%, an unsurprising result given that several of the elements in the expanded set of controls might be endogenous. What is reassuring, is that the magnitude of the parameter estimates is very similar when including this expanded set of controls.

Now we turn the light to the results using the enclave instrument which is common in the labor and migration literature. Again, Appendix Table A.2 provides the first-stage F, this time in the second column. The Enclave instrument is highly significant and the first-stage F is even higher than that for Receptivity, which is as expected given our discussion in Section III. Column (3) includes both instruments, showing that both instruments have independent predictive power for inflows. Table 4 shows the IV results using the Enclave instrument following the same order than in the previous IV and OLS tables. We confirm a positive and significant relationship between inflows and homicides. Nicely, the parameter estimates are similar in magnitude and significance than those using the Receptivity instrument and we cannot reject the null of equality of coefficients across the two models. Our baseline specification using the Enclave instrument generates a coefficient of 7.934 while the analogous specification using Receptivity generates a coefficient of 7.129.

Table 4—: Homicides and IDP Inflows. IV using Enclave

	(1)	(2)	(3)	(4)
IDP Inflows t-1	3.034***	7.723***	7.934***	8.926***
	(0.968)	(1.158)	(1.189)	(0.808)
Observations	66045	66045	62184	58183
Standarized Coefficients	0.242	0.615	0.627	0.694
F-Statistic	494.9	387.6	374.9	358.8
Municipality FE	Y	Y	Y	Y
Time FE	Y	Y	Y	Y
Municipality-specific Linear Trend	N	Y	Y	Y
Time Dummies Interacted with Remoteness	N	N	Y	Y
Expanded Set of Controls	N	N	N	N

Standard errors clustered at the municipality level in parentheses. The dependent variable is the homicides rate per 100,000 inhabitants. All non-dichotomous and non-count variables expressed in natural logarithms. All regressions are weighed by population. Expanded set of controls: population, tax revenues from industry and commerce, public school teachers per student, percentage of the population aged 15 to 39, rurality index, plus the unemployment rate in 1993 and the count of military bases in 2007, both time interacted.

The similarity of IV estimates suggests that our estimates do a good job in washing out the endogenous component of inflows. Nevertheless, we gauge the sensitivity of our IV-Receptivity regressions by excluding outflows from nearby municipalities. The benefit of doing this is that nearby municipalities might be similar in unobservables and excluding them might reduce concerns about spatial correlation in violence. However, this comes also with a high cost since IDP move to close municipalities. Table 5 show the results of re-estimating our baseline specification excluding from Receptivity outflows within 25, 50, 75, 100 and 125 kilometers from the recipient municipality.

Recall that $\hat{\beta}$ is equal to 7.129 when we use all outflows (column 3 of table 3). The main parameter estimate decreases as the distance threshold increases, a pattern that is consistent with overestimation due to spatial correlation in violence. However note that when we remove municipalities within 75, 100, and 125 km our instrument becomes weak, with the first-stage F falling well below 10 for the cases of 100 and 125km. In terms of significance, standard errors increase exponentially as we remove more municipalities. Similarly, first state F-statistics fall rapidly. We have marginal significant results when excluding municipalities within 50 km ($p - value = 0.108$). In that case we exclude, on average, 10.4 surrounding municipalities. When removing more municipalities, our estimates become highly imprecise. To conclude, our estimates are robust to excluding nearby municipalities falling within 50 KM of the host municipality. However, parameter estimates remain positive and quantitatively important even when removing outflows within 75 KM which boils down to excluding on average 22 municipalities. These results, together with the similarity between the IV-Receptivity and the IV-Enclave estimates, assuage endogeneity concerns.

Table 5—: Homicides and IDP Inflows. IV using receptivity and excluding nearby outflowss

	(1) Threshold: 25Km	(2) Threshold: 50Km	(3) Threshold: 75Km	(4) Threshold: 100Km	(5) Threshold: 125Km
IDP Inflows t-1	6.159***	5.085	3.424	0.0884	-18.77
	(2.311)	(3.206)	(4.549)	(5.376)	(28.10)
Observations	65467	65467	65467	65467	65467
Standarized Coefficients	0.489	0.404	0.272	0.00702	-1.490
Excluded Municipalities (average)	2.886	10.39	21.71	36.05	53.08
F-Statistic	57.07	29.58	11.34	3.444	0.354

Standard errors clustered at the municipality level in parentheses. The dependent variable is the homicides rate per 100,000 inhabitants. All non-dichotomous and non-count variables expressed in natural logarithms. All regressions are weighed by population and include fixed effects at the municipality and time level as well as municipality-specific linear trends and time dummies interacted with remoteness.

To close this section, we examine the impact of IDP inflows on homicides over time. To do this we estimate equations like (1) replacing $ln\left(Inflows_{c,t-1}\right)$

by $ln\left(Inflows_{c,t-\tau}\right)$ for $\tau = 1, 2, ..., 20$ (i.e., we run 20 separate regressions).

Figure 3. : IDP Inflows and Homicides over time

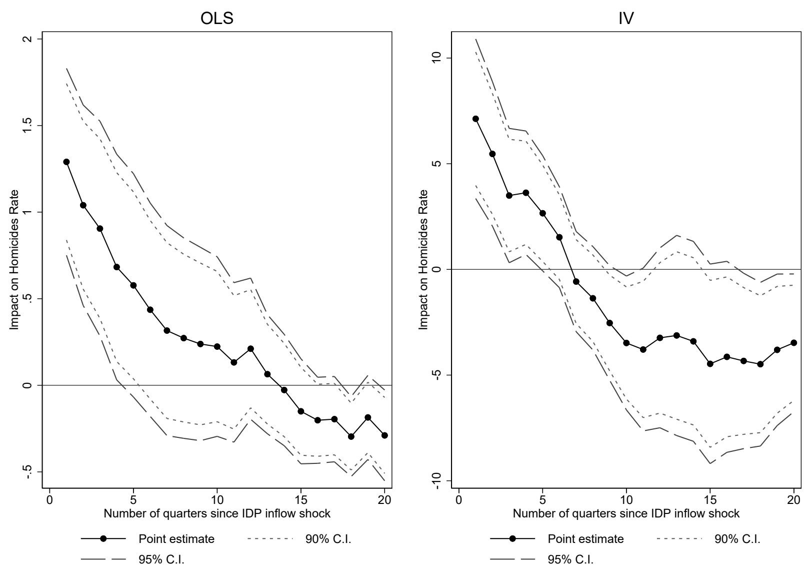


Figure 3 show graphs summarizing these estimations. The left graph shows OLS results, the right graph shows IV results. Both graphs suggest that the most important lag of inflows is that in $t-1$, which is the justification of using this lag in the econometrics. Furthermore, the figures support the idea that the impact of IDP inflows on homicides is short lived: inflows in $t-1$ and up to $t-5$ affect homicides. This is an important result because it suggest that recent —within a year— arrivals of IDP affect violence. The initial disruptions originated by large population shocks might be behind spikes in homicides.

V. Refinements

In this subsection we analyze the details behind the effect of forced migration on homicides. In particular, we ask if IDP inflows affect differentially non-permanent residents and permanent residents, we ask whether the effect on homicides is larger in urban areas and how the effect varies by the gender and the age group of the deceased. Finally, we provide falsification tests that show that natural deaths—also recorded in the vital statistics data— are not affected by IDP inflows.

First, we have to stress that our results do not allow us to infer who is the victim and who is the victimizer. In other words, we do not know who is committing the homicides among the displaced and non-displaced population. This is a data restriction because the vital statistics do not indicate if the homicide represents the death of a displaced person or not. However, we can refine our analysis by distinguishing homicides of permanent residents and of people who usually reside in another municipality. This information is available in the vital statistics data since relatives of defuncts report it. It is likely that the fraction of IDP is higher among the people who usually reside in another municipality but we cannot make this assertion without ambiguity.

With that being said, Table 6 displays OLS and IV-Receptivity regressions for permanent-residents (columns 1 and 2, respectively) and for non-permanent residents (i.e.: People who usually reside in another municipality; columns 3 and 4, respectively). Before proceeding with the results, homicides of non-permanent residents represent 35.6% of total homicides. Migrants represent a similar fraction of total population. Thus, migrants as measured by non-permanent residents do not seem to be over- or under-represented in the homicides data.

Turning to the results in table 6, the results presented in the previous section seem to be mostly explained by homicides of permanent residents. The magnitude and significance of the OLS and IV coefficients for permanent residents are smaller in magnitudes but close to that found in tables 2 and 3 above. The point estimates for non-permanent residents are three time smaller (either in the OLS or the IV case). We interpret these results as follows: permanent residents (non-IDP or established IDP) are those who suffer more from IDP inflows. This might be suggestive evident that recent IDP do not seem to be the victims of violence as long as IDP deaths are likely to be classified as residents of other municipalities. This is surprising if we recall that IDP inflows have a short-lived impact on homicides.

Table 6—: Homicides and IDP Inflows. Distinguishing between permanent residents and people who usually reside in another municipality. IV-Receptivity.

	(1) Permanent Residents	(2) Permanent Residents	(3) Non-Permanent Residents	(4) Non-Permanent Residents
IDP Inflows t-1	0.966***	5.299***	0.325***	1.831**
	(0.226)	(1.250)	(0.0654)	(0.790)
Observations	65467	65467	65467	65467
Standardized Coefficient	0.114	0.624	0.0444	0.250
F-Statistic	.	72.82	.	72.82

Standard errors clustered at the municipality level in parentheses.The dependent variable is the homicides rate per 100,000 inhabitants. All non-dichotomous and non-count variables expressed in natural logarithms. All regressions are weighed by population and include fixed effects at the municipality and time level as well as municipality-specific linear trends and time dummies interacted with remoteness.

In what follows we focus on IV regressions (using the Receptivity instrument) and refrain from presenting OLS estimates for conciseness. We first look for differential effects of IDP in cities and in non-urban areas. IDP arrive in larger numbers to cities and cities seem to have problems absorbing them: for instance, IDP inflows lower the wages of unskilled workers (Calderón-Mejía and Ibáñez, 2015) and increase low income rental prices in cities (Depetris-Chauvin and Santos, 2018). Table 7 estimates regressions for department capitals (column 1), municipality seats (column 2) and non-municipality seats (columns 3). Standardized coefficient reveal that the effect is larger an monotonically decreasing for more populous areas.

Table 7—: Homicides and IDP Inflows in principal cities, urban areas and rural areas.

	(1) Department Capitals	(2) Municipality Seats	(3) Non-Municipality Seats
IDP Inflows t-1	9.813**	3.241*	3.775***
	(4.325)	(1.664)	(0.987)
Observations	1876	65467	65467
Standardized Coefficient	1.820	0.673	0.351
F-Statistic	6.479	72.82	72.82

Standard errors clustered at the municipality level in parentheses. All non-dichotomous and non-count variables expressed in natural logarithms. All regressions are weighed by population and include fixed effects at the municipality and time level as well as municipality-specific linear trends and time dummies interacted with remoteness.

Table 8 provides a test which is common in the literature of crime. It estimates separately the effect of IDP inflows on the murder rate for males and for females (respectively, in column 1 and 2). Since men are more likely to engage in violent activities which might result in murders, we expect the effect to be higher for males. Table 8 shows that the effect of IDP inflows on homicides is mainly operative through male homicides: The standardized coefficient for males is 2.2 that for females.

Table 9 presents results dividing the homicides rate by 5 years age groups. Three main results arise from this table: First, homicides in all age groups are affected by IDP inflows. Second, the largest standardized effects are for the 15-19 and 20-25 age groups. Third, these standardized effects rapidly and monotonically decrease after the 40-44 age group. However, people in

Table 8—: Homicides and IDP Inflows by gender

	(1) Male	(2) Female
IDP Inflows t-1	6.615***	0.511***
	(1.796)	(0.157)
Observations	65467	65467
Standardized Coefficient	0.573	0.225
First-stage F	72.82	72.82

Standard errors clustered at the municipality level in parentheses. All non-dichotomous and non-count variables expressed in natural logarithms. All regressions are weighed by population and include fixed effects at the municipality and time level as well as municipality-specific linear trends and time dummies interacted with remoteness.

the age group 60 to 64 are also highly affected by violence as measured by homicides and people in this age group are unlikely to be IDP.

Table 9—: Homicides and IDP Inflows by age groups

	(1) 10-14	(2) 15-19	(3) 20-24	(4) 25-29	(5) 30-34	(6) 35-39	(7) 40-44	(8) 45-49	(9) 50-54	(10) 55-59	(11) 60-64
IDP Inflows t-1	0.0987**	1.081**	1.593***	0.944***	0.928***	0.574***	0.499***	0.418***	0.318***	0.218***	0.143***
	(0.0464)	(0.459)	(0.536)	(0.310)	(0.250)	(0.181)	(0.130)	(0.0937)	(0.0653)	(0.0548)	(0.0356)
Observations	65467	65467	65467	65467	65467	65467	65467	65467	65467	65467	65467
Standardized Coefficient	0.190	0.552	0.445	0.259	0.343	0.250	0.261	0.268	0.230	0.202	0.159
First-stage F	72.82	72.82	72.82	72.82	72.82	72.82	72.82	72.82	72.82	72.82	72.82

Standard errors clustered at the municipality level in parentheses. All non-dichotomous and non-count variables expressed in natural logarithms. All regressions are weighed by population and include fixed effects at the municipality and time level as well as municipality-specific linear trends and time dummies interacted with remoteness.

To finish, Table 10 presents a simple falsification test where we replicate our main IV table but replacing the homicides rate by the number of natural deaths per 100000 inhabitants. If health is not severely affected by the arrival of IDP (we are not aware of evidence of this for Colombia) and if the homicides result is not driven by some peculiarity of the vital statistics data, we should expect no relationship between natural deaths and IDP inflows. Reassuringly, the relationship between IDP inflows and natural deaths is insignificant for our preferred specifications (columns 3 and 4) [4]. This allows us to conclude that —conditional on time and municipality fixed effects, and municipality time trends— our results on homicides do not seem to be generated by some peculiarity of the vital statistics data like under-reporting in places that receive less IDP (i.e.: remote rural areas of Colombia).

Table 10—: Homicides and IDP Inflows and Natural Deaths

	(1)	(2)	(3)	(4)
IDP Inflows t-1	-2.757*	1.953	1.828	-0.168
	(1.492)	(1.968)	(1.979)	(1.798)
Observations	65467	65467	65467	58183
Standarized Coefficients	-0.116	0.0822	0.145	-0.00718
F-Statistic	236.4	96.44	72.82	92.89
Municipality FE	Y	Y	Y	Y
Time FE	Y	Y	Y	Y
Municipality-specific Linear Trend	N	Y	Y	Y
Time Dummies Interacted with Remoteness	N	N	Y	Y
Expanded Set of Controls	N	N	N	Y

Standard errors clustered at the municipality level in parentheses. The dependent variable is the natural deaths rate per 100,000 inhabitants. All non-dichotomous and non-count variables expressed in natural logarithms. All regressions are weighed by population. Expanded set of controls: population, tax revenues from industry and commerce, public school teachers per student, percentage of the population aged 15 to 39, rurality index, plus the unemployment rate in 1993 and the count of military bases in 2007, both time interacted.

VI. Conclusions

We leverage a novel dataset with high quality administrative panel data on quarterly IDP flows and vital statistics on deaths across Colombian Municipalities to identify the causal impact of IDP inflows on homicide rates across Colombian municipalities during the period 1999-2014. Given

[4]Although in column 1, where we include time and municipality fixed effects, the parameter estimate is negative and significant at the 10% level.

its quality, high frequency, and temporal extension, our data provides a meaningful source of variation in IDP flows to identify the main effect of interest. Indeed, unlike previous works which used different proxies for the intensity of IDP inflows due to the lack of IDP data at the host level, our paper exploits actual IDP figures at the municipality level. Using high-frequency (i.e., quarterly) data mitigates concerns of other time-varying factors that may take place at longer time intervals (e.g., annually) and may potentially confound our analysis. Moreover, the time period under analysis provides time windows before, during, and after the peak of the displacement crisis occurred in the early 2000's due to the intensification of the Colombian internal conflict.

Following different instrumental variables strategies to address the potential endogeneity of the location choices of IDP, we find that IDP inflows positively impact homicides. The effect is economically important: a standard deviation increase in IDP inflows is associated with a 0.6 standard deviations increase in the homicides rate. This effect is larger in cities and among men. While our data does not allow us to infer who is the victim and who is the victimizer (i.e.: we do not know who is committing the homicides among the displaced and non-displaced population), we refine our analysis by distinguishing homicides of permanent residents and of people who usually reside in another municipality. We find that our results are mostly explained by homicides of permanent residents. We also find that the standardized effects are larger for young individuals (i.e., age groups 15-19 and 20-24). We also found that the impact of IDP inflows on homicides is short lived, lasting at most 5 quarters. Combining this result with previous research that shows that IDP inflows reduce wages (Calderón-Mejía and Ibáñez, 2015) and increase low-income rental prices (Depetris-Chauvin and Santos, 2018), policy makers should focus their efforts in reducing the negative impacts that recent IDP arrivals have on local economies. Reducing coexistence problems with the local population is also a must when people who did not freely chose to leave their homes arrive to a new and difficult environment.

Appendix: Additional Tables

Table A.1—: Data Description

Variable	**Definition**	**Source**
	A. Dependent Variables	
Homicides Rate	Total homicides per 100,000 inhabitants. We also disaggregate homicide rates by gender, aging group, zone and migration status. Time Period: 1998-I to 2014-IV.	*Vital Statistics, DANE.*
Natural Deaths Rate	Total natural deaths per 100,000 inhabitants. Time Period: 1998-I to 2014-IV.	*Vital Statistics, DANE.*
Suicides Rate	Total suicides per 100,000 inhabitants. Time Period: 1998-I to 2014-IV.	*Vital Statistics, DANE.*
Traffic Accidents Rate	Total traffic accidents per 100,000 inhabitants. Time Period: 1998-I to 2014-IV.	*Vital Statistics, DANE.*
	B. Independent Variables	
IDP Inflows	Inflows of internally displaced persons (IDPs) received by host cities. Measured quarterly from 1997-I to 2014-IV.	*Victims' Register (Registro Único de Víctimas (RUV)).*
Receptivity Instrument	Weighed sum of the IDP outflows from all Colombian municipalities (except the host city), where the weights are (the inverse of) either the road or geodesic distances between the expelling municipalities and the host city. Time Period: 1998-I to 2014-IV.	*Outflows from Victims' Register (Registro Único de Víctimas (RUV)) and road distances from SIGOT-IGAC.*
Enclave Instrument	Weighed sum of the IDP outflows from all Colombian municipalities (except the host city), where the weights are share of migrants of the expelling municipalities who lived in the host city in 1993. Time Period: 1998-I to 2014-IV.	*Outflows from Victims' Register (Registro Único de Víctimas (RUV)) and the share of migrants from National Census of 1993, DANE.*
	C. Controls	
Population	Total municipal population (in logs). Time Period: 1998 to 2014.	*Population Projections based on National Census of 1993 and 2005, DANE.*
Tax Revenues	Municipal tax revenues from industry and commerce (in logs). Time Period: 1998 to 2014.	*Municipal Panel , CEDE - Universidad de los Andes.*
Public School Teachers per Student	Ratio of public school teachers to students (in logs). Time Period: 1998 to 2014.	*Municipal Panel , CEDE - Universidad de los Andes.*
Average Road Distance	Average road distance from the host city to all other Colombian municipalities (in logs).	*SIGOT-IGAC.*
Share of Population between 15 and 39	Fraction of total population that is between 15 and 39 years old (in logs). Time Period: 1998 to 2014.	*Population Projections based on National Census of 1993 and 2005, DANE.*
Rural Index	Fraction of municipality's population that lives in rural area(in logs). Time Period: 1998 to 2014.	*Municipal Panel , CEDE - Universidad de los Andes.*
Unemployment Rate	Municipal unemployment rate (in logs). Time Period: 1993.	*National Census of 1993, DANE.*
Military Bases	Number of military bases (army, air force and navy)	*Acemoglu et al.(2016) - Colombian Army Official Website*

Table A.2—: First stages: Receptivity and Enclave instruments

	(1)	(2)	(3)
Receptivity Instrument	4.601***		2.806***
	(0.539)		(0.450)
Enclave Instrument		0.582***	0.509***
		(0.0474)	(0.0483)
Observations	65467	62184	62184
F-Statistic	72.82	151.0	121.6

Standard errors clustered at the municipality level in parentheses. All non-dichotomous and non-count variables expressed in natural logarithms. All regressions are weighted by city population. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

REFERENCES

Acemoglu, D., J. A. Robinson, and R. J. Santos (2013). The monopoly of violence: Evidence from colombia. *Journal of the European Economic Association 11*, 5–44.

Becker, G. S. (1968). Crime and punishment: An economic approach. *Journal of Political Economy 76*(2), 169–217.

Bell, B., F. Fasani, and S. Machin (2013). Crime and immigration: Evidence from large immigrant waves. *The Review of Economics and Statistics 95*(4), 1278–1290.

Bianchi, M., P. Buonanno, and P. Pinotti (2012). Do immigrants cause crime? *Journal of the European Economic Association 10*(6), 1318–1347.

Bohada, M. d. P. (2010). Forced Displacement and Living Conditions of Receiving Communities: The Case of Pasto, Nariño. *Revista de Economía Institucional 12*(23), 259.

Borjas, G. J., R. B. Freeman, L. F. Katz, J. DiNardo, and J. M. Abowd (1997). How much do immigration and trade affect labor market outcomes? *Brookings Papers on Economic Activity 1997*(1), 1–90.

Calderón-Mejía, V. and A. M. Ibáñez (2015). Labour market effects of migration-related supply shocks: evidence from internal refugees in Colombia. *Journal of Economic Geography 49*(6), 772–784.

Card, D. (1990). The impact of the mariel boatlift on the miami labor market. *Industrial and Labor Relations Review 43*(2), 245–257.

Card, D. (2001, January). Immigrant Inflows, Native Outflows, and the Local Labor Market Impacts of Higher Immigration. *Journal of Labor Economics 19*(1), 22–64.

Centro de Estudios sobre Desarrollo Economico (2004). Encuesta de hogares desplazados. Data extracted from `https://datoscede.uniandes.edu.co/datoscede/microdatos-detalle.php/259/encuesta-de-hogares-desplazados/`.

Centro de Estudios sobre Desarrollo Económico (2015). Panel municipal del cede. Data retrieved from `https://datoscede.uniandes.edu.co/microdatos-detalle.php/263/panel-municipal-del-cede/`.

Couttenier, M., V. Preotu, D. Rohner, and M. Thoenig (2016, January). The Violent Legacy of Victimization: Post-Conflict Evidence on Asylum Seekers, Crimes and Public Policy in Switzerland. CEPR Discussion Papers 11079, C.E.P.R. Discussion Papers.

Departamento Administrativo de Planeación Distrital (2004). El desplazamiento en bogotá: Una realidad que clama atención. Technical report, Alcaldía Mayor de Bogotá.

Depetris-Chauvin, E. and R. Santos (2018). Unexpected guess: The impact of internal displacement inflows on rental prices in colombian host cities.

Ehrlich, I. (1973). Participation in illegitimate activities: A theoretical and empirical investigation. *Journal of Political Economy 81*(3), 521–565.

Grossman, J. B. (1982). The substitutability of natives and immigrants in production. *The Review of Economics and Statistics 64*(4), 596–603.

Ibáñez, A. (2008). *El desplazamiento forzoso en Colombia: Un camino sin retorno hacia la pobreza*. Editorial Uniandes.

Morales, J. S. (2018). The impact of internal displacement on destination communities: Evidence from the Colombian conflict. *Journal of Development Economics 131*, 132 – 150.

Saiz, A. (2007). Immigration and housing rents in american cities. *Journal of Urban Economics 61*(2), 345–371.

Valenzuela, S. (2014, Oct). Epidemia de violencia en San Cristóbal. *El Espectador*.

Varano, S. P., J. A. Schafer, J. M. Cancino, S. H. Decker, and J. R. Greene (2010). A tale of three cities: Crime and displacement after Hurricane Katrina. *Journal of Criminal Justice 38*(1), 42 – 50.